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A Fine Time for Monetary Policy?

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Almost everyone would agree—even we in the Federal Reserve System—that monetary policy can be improved. But improving it requires accurate empirical descriptions of the current policy and the relationship between that policy and the economic variables policymakers care about. With those descriptions, we could, conceivably, predict how economic outcomes would change under alternative policies and hence find policies that lead to better economic outcomes.

The first requirement of this policymaking problem is policy identification, and it is the focus of this study. Policy identification entails a specification of the instrument the Federal Reserve controls and a description of how that instrument is set based on information available when a policy decision is made. Because policy identification is a crucial step in the search for improved monetary policy, it has received much attention in the literature.

Although many different approaches have been taken to identify monetary policy, all are potentially tainted by time-aggregation problems. All studies use data averaged over periods of a month or more. Yet financial variables in the Fed's province move and interact essentially minute by minute. So one might suspect that averaging data over periods of a month or more would obscure how variables like reserves and interest rates interrelate over time. For instance, if a change in reserves leads shortly to a change in interest rates and that change then feeds back onto reserves, and so on, a model using data as coarse as monthly or quarterly cannot uncover these finer-time lead-lag relationships.

This study investigates the sensitivity of conclusions about monetary policy to the specification of period length. We identify a model including total reserves, nonborrowed reserves, and the federal funds rate, based on our understanding of the Fed's operating procedures. We indicate some further, testable restrictions the model should satisfy if that understanding is correct. We estimate the model using data for biweekly periods (measured every two weeks) as well as for data averaged over quarterly periods.

Our study suggests that time aggregation from a biweekly interval to a quarterly interval is not a problem when identifying monetary policy. And time aggregation does not seem to be a problem when evaluating the dynamic effects of typical changes in variables. This is fortunate because other time series like output, employment, and prices are not available biweekly and some measures are available only quarterly. In policy identification and evaluation, time aggregation is not a major concern.

We offer the following explanation for our counterintuitive findings. Two types of disturbances occur in reserve markets. One is high-frequency noise, which has only transitory effects upon the monetary variables. The other consists of low-frequency monetary policy changes, which are persistent and have persistent effects. Time-aggregated models, then, can be used for policy identification because they filter out the noise but retain the policy changes and their effects.

Policy Identification

Policy identification plays an important role in the search for better policies. Time-aggregation problems may confound the identification, but need not do so. In order to make these arguments concrete, we posit a simple conceptual model. This model also serves to motivate the empirical investigation that follows.

In our simple model, we suppose that the Fed conducts monetary policy by determining the supply of banks' nonborrowed reserves *NR*.[1] The Fed's policy aims to achieve some goal, which we suppose can be measured in terms of a variable *GV*, such as payroll employment, the consumer price index, gross domestic product (GDP), or the GDP deflator. We also suppose that the Fed's supply of nonborrowed reserves is affected by the level of banks' total reserves *TR* and that *TR* depends both on past policy actions and on economic activity as captured by *GV*. Moreover, we suppose that policy actions interact with the banks' demand for nonborrowed reserves to determine the federal funds rate *FF*, which then feeds through to affect economic activity and hence *GV*. Thus our simple model consists of the four time series *GV*, *TR*, *NR*, and *FF*.

If we were using this model to search for improved policies, we would have to correctly identify past policy. In order to see why, let us describe the identification problem. (Here we closely follow the development in Faust and Leeper 1994.)

If we transform each element of our time series $X_t \equiv \langle GV_t, TR_t, NR_t, FF_t \rangle'$ to be stationary and call the transformed variables $X_t^* \equiv \langle GV_t^*, TR_t^*, NR_t^*, FF_t^* \rangle'$, X_t^* has a moving-average representation:[2]

$$(1) \qquad X_t^* = m + M(L)u_t.$$

In this representation, m is a 4×1 vector and the vector $u_t \equiv \langle u_{1t}, u_{2t}, u_{3t}, u_{4t} \rangle'$ has mean zero, is serially uncorrelated, and has a time-invariant variance-covariance matrix, $E(u_t u_t') = V$ for all t. The term $M(L)$ is defined by $M(L) \equiv \Sigma_{k=0}^{\infty} M_k L^k$, where for each k, M_k is a 4×4 matrix of coefficients and $L^k X_t^* \equiv X_{t-k}^*$. Thus the moving-average representation has the following form:

$$(2) \qquad X_t^* = m + M_0 u_t + M_1 u_{t-1} + M_2 u_{t-2} + \dots.$$

We will also assume that $M(L)$ is invertible, so that our model also has a (vector) autoregressive representation:

$$(3) \qquad A(L)X_t^* = a + u_t$$

where $A(L) = M(L)^{-1}$ and $a = (\Sigma_{k=0}^{\infty} A_k)m$.

Although a straightforward procedure to follow in the search for better policies does exist, that procedure is undone by identification problems. Under the straightforward approach, lag lengths would be made finite, the finite-lag version of equation (3) would be estimated, the estimated version of the nonborrowed reserves equation would be taken as the historical policy rule, and alternative specifications of this equation would be evaluated in terms of the desirability of the outcomes they imply for the goal variable.

To explain why this procedure is not valid, we note that the original moving-average representation for X_t^* is not unique.[3] Take any invertible 4×4 matrix D and note that

$$(4) \qquad X_t^* = M(L)DD^{-1}u_t = B(L)\varepsilon_t$$

where $B(L) \equiv M(L)D$ and $\varepsilon_t \equiv \langle \varepsilon_{1t}, \varepsilon_{2t}, \varepsilon_{3t}, \varepsilon_{4t} \rangle' \equiv D^{-1}u_t$. Since $M(L)$ was assumed to be invertible, $B(L)$ will also be invertible, and thus X will have the observationally equivalent (vector) autoregressive form

$$(5) \qquad C(L)X_t^* = \varepsilon_t$$

where $C(L) \equiv B(L)^{-1}$.

Identification of our model requires that we impose enough restrictions on $M(L)$ or $A(L)$ that the only invertible matrix D for which $B(L) = M(L)D$ and $C(L) = D^{-1}A(L)$ satisfy the restrictions is $D = I$, thus making the representations unique. One way to approach identification is to sort out the contemporaneous shocks $\varepsilon_t = D^{-1}u_t$. In the particular case of the total reserves equation, identification should ensure that ε_{3t} is a policy shock and not a conglomeration of other shocks. We can see that the choice of D—call it D_0—determines the variance-covariance matrix of the shocks ε_t by

$$(6) \qquad E(\varepsilon_t\varepsilon_t') = E(D_0^{-1}u_tu_t'D_0^{-1\prime}) = D_0^{-1}E(u_tu_t')D_0^{-1\prime} = D_0^{-1}VD_0^{-1\prime}.$$

We refer to *policy identification* in our model as restricting the elements of $B_0(\equiv M_0D_0)$ to ensure that ε_{3t} is a pure policy shock, uncorrelated with the shocks to the other variables in the model. Only if the policy has been identified will the estimated coefficients of the nonborrowed reserves equation represent the historical policy rule. Moreover, if policy has been identified incorrectly—that is, if B_0 has been misspecified—then the effects of a surprise policy action mistakenly taken to be ε_{3t} will be found to have incorrectly quantified effects on the other variables.

Since policy identification is so central to the empirical search for better policies, researchers have obviously focused much attention on this problem and used many different approaches to solve it.[4] For exposition, we adopt an identification scheme for our model that is consistent with our understanding of the monetary policymaking process.

Our identified model sorts out contemporaneous shocks by adopting a particular ordering. We assume that our goal variable (GV) is first in the ordering. That is, the current value of the goal variable depends on past values of other variables but not on their current values. If we suppress lagged values for now, we can express our identified equation as

$$(7) \qquad GV_t^* = a_{10} + \varepsilon_{1t}.$$

We next assume that the second variable in our ordering, total reserves (TR), is affected by past policy actions and by past and present levels of economic activity as captured in levels of the goal variable. Our identified equation (with lags suppressed) is

$$(8) \qquad TR_t^* = a_{20} + a_{21}GV_t^* + \varepsilon_{2t}.$$

We take nonborrowed reserves (NR) to be the third variable in the ordering and assume it represents Fed policy. We suppose the Fed can react to the current value of the goal variable and try to steer it to its desired path. The Fed's action on nonborrowed reserves also may depend on the movement in total reserves that it takes as outside its control in the current period. Our identified policy (again, with lags suppressed) is

$$(9) \qquad NR_t^* = a_{30} + a_{31}GV_t^* + a_{32}TR_t^* + \varepsilon_{3t}.$$

We note that we can rewrite (9) as

$$(10) \qquad NR_t^* = a_{30}' + a_{31}'GV_t^* + a_{32}(TR_t^* - \overline{TR}_t) + \varepsilon_{3t}$$

where $\overline{TR}_t \equiv a_{20} + a_{21}GV_t^*$. Then a_{32} measures the response of nonborrowed reserves to unforeseen shocks to total reserves. If a_{32} is zero, the Fed does not accommodate shocks to total reserves, so that those shocks imply equal changes to borrowed reserves. If a_{32} is one, the Fed accommodates shocks to total reserves with like changes in nonborrowed reserves. Finally, we suppose that the current federal funds rate (FF), the fourth variable in our ordering, depends on current and past economic conditions as captured by the levels of the goal variable and on current and past levels of (approximately) borrowed reserves. So (with lags suppressed) our fourth identified equation is

$$(11) \qquad FF_t^* = a_{40} + a_{41}GV_t^* + a_{42}TR_t^* + a_{43}NR_t^* + \varepsilon_{4t}$$

with $a_{42} \cong -a_{43}$.

If $a_{42} > 0$ and $a_{43} = -a_{42}$, then the shock ε_{4t} affects the demand for borrowed reserves. An increase in ε_{4t} thus raises the federal funds rate. In the scheme, ε_{3t} is a shock to the Fed's supply of nonborrowed reserves. An increase in ε_{3t} thus lowers the federal funds rate (which is a liquidity effect).[5] The effects of a shock to total reserves ε_{2t} will depend on whether or not the Fed accommodates the demand shock.

The relations (8) through (11) and some of the implied interactions between the markets for reserves are shown in Charts 1–3. These charts show the effects of a positive shock to the demand for total reserves (8). The effects of a change in the goal variable, such that $a_{21}\Delta GV_t^* > 0$, would be qualitatively the same. If the Fed does not accommodate (that is, $a_{32} = 0$), then the entire increase in TR_t^* must come about through an increase in borrowings $TR_t^* - NR_t^*$, which raises the federal funds rate in the market for nonborrowed reserves. If the Fed accommodates completely (that is, $a_{32} = 1$), then the borrowings and the funds rate remain unchanged. If the Fed partially accommodates (that is, $0 < a_{32} < 1$), then NR_t^*, borrowings, and the funds rate all increase. Our understanding of the Fed's operating procedure is that it targets borrowed reserves (that is, $a_{32} = 1$) because it thinks that a close concurrent relationship exists between borrowed reserves and the federal funds rate (that is, $a_{42} > 0$ and $a_{43} = -a_{42}$). We will later examine our estimates to see whether these restrictions are borne out.

If the lagged variables are reintroduced, our identified model is

$$(12) \qquad GV_t^* = a_{10} + \sum_{k=1}^{\infty} a_{11k}GV_{t-k}^* + \sum_{k=1}^{\infty} a_{12k}TR_{t-k}^* + \sum_{k=1}^{\infty} a_{13k}NR_{t-k}^* + \sum_{k=1}^{\infty} a_{14k}FF_{t-k}^* + u_{1k}$$

$$(13) \qquad TR_t^* = a_{20} + \sum_{k=0}^{\infty} a_{21k}GV_{t-k}^* + \sum_{k=1}^{\infty} a_{22k}TR_{t-k}^* + \sum_{k=1}^{\infty} a_{23k}NR_{t-k}^* + \sum_{k=1}^{\infty} a_{24k}FF_{t-k}^* + u_{2k}$$

$$(14) \qquad NR_t^* = a_{30} + \sum_{k=0}^{\infty} a_{31k}GV_{t-k}^* + \sum_{k=0}^{\infty} a_{32k}TR_{t-k}^* + \sum_{k=1}^{\infty} a_{33k}NR_{t-k}^* + \sum_{k=1}^{\infty} a_{34k}FF_{t-k}^* + u_{3k}$$

$$(15)\quad FF_t^* = a_{40} + \sum_{k=0}^{\infty} a_{41k} GV_{t-k}^* + \sum_{k=0}^{\infty} a_{42k} TR_{t-k}^* + \sum_{k=0}^{\infty} a_{43k} NR_{t-k}^* + \sum_{k=1}^{\infty} a_{44k} FF_{t-k}^* + u_{4k}.$$

Time Aggregation

So far, we have argued that policy identification is important to policy evaluation and that the identification scheme for our simple model seems reasonable. However, we have not considered the time interval over which the model's variables should be measured. Should the variables be averages over a quarter, a month, or something finer? Does the choice of time period length affect the estimates of the identified policy rule?

Faust and Leeper (1994) formally examine the relationship between time aggregation and identification. They show that very stringent conditions are necessary for time aggregation to avoid having any effect in a time series model. Their conditions are surely violated in our four-variable model. So we concentrate on evaluating the significance of time aggregation in identifying the effect of monetary policy.

In an ideal world, we would have data on all the variables in our model at the highest frequency that interested us—daily, weekly, or biweekly (measured every two weeks)—and we could directly estimate how time aggregation influences the estimated effect of a policy shock by estimating our model separately with data constructed with each different level of time aggregation. We could then directly test whether, for example, the estimated effect of a policy shock in a model using weekly data was the same as an equivalent policy shock in a model using quarterly data.

Unfortunately, while daily data are available on the federal funds rate, only biweekly data are available on total reserves and nonborrowed reserves. These biweekly data are the average reserve level over each *reserve maintenance period*—the period over which required reserves are computed for banks.[6] Data on potential goal variables are available only monthly for employment and the consumer price index and quarterly for GDP and the GDP deflator.

If we tried to estimate a model including total reserves, nonborrowed reserves, the federal funds rate, and GDP, for example, we could only use quarterly data since that is the highest frequency with which GDP data are measured. As a consequence, estimating a full model with four variables tells us nothing about time aggregation because we must estimate the full model with time-aggregated data.

Because of this problem, we test for time-aggregation problems using high-frequency data on total reserves, nonborrowed reserves, and the federal funds rate. Since each of these variables is available at least biweekly, we can determine whether time aggregation is important within this submodel of the larger model by estimating the submodel using both biweekly (2-week) and quarterly (12-week) data and comparing those estimates to determine whether time aggregation matters within the submodel.[7] Because interest rates respond very quickly to changes in other variables, we expect that if time-aggregation problems exist, they are most likely to show up in this three-variable model. Thus our three-variable model essentially consists of equations (13)–(15) with the $a_{\cdot 1}$ coefficients set to zero.

This evaluation of the effect of time aggregation is less direct than the corresponding exercise in the four-variable model with complete data for all periods. In the four-variable model, we showed that a policy shock could be identified with the shock ε_{3t} of the nonborrowed reserves equation. Therefore, if we could estimate that model using different data frequencies, we would only need to look at the effect of a shock to nonborrowed reserves on the other variables to measure the effect of a policy. Comparing those estimates with data of different frequencies would allow us to assess the effect of time aggregation. Such a simple assessment of the effect of time aggregation is not possible using the submodel, because policy cannot be identified with a shock to any particular component of a three-variable model, precisely because the three-variable model omits the goal variable.

What, then, can we learn from a three-variable model? We can discover whether time aggregation significantly affects our assessment of the dynamics of the three-variable model. Even if we cannot identify a shock to one of the variables in the three-variable model as a policy shock, we can still study whether our assessment of the three-variable model's dynamics depends on if we estimate it using 2-week data or 12-week data. If important differences exist between these estimates, then time aggregation is significant in the three-variable model; it would also be significant in the full model, if we had 2-week data on the goal variable.

The Model and Experiments

As mentioned above, the three-variable vector autoregression (VAR) we examine includes total reserves, nonborrowed reserves, and the federal funds rate. The exact specification of variables used follows Strongin 1992.

As in Strongin 1992, here the total reserves variable is total reserves in the current period divided by the level of total reserves in the previous period, and the nonborrowed reserves variable is nonborrowed reserves in the current period divided by the level of total reserves in the previous period. (As mentioned in footnote 2, we use this transformation to induce stationarity in the series.) The VAR is organized so that six months of lags of all variables appear as explanatory variables in each of the three equations. The nonborrowed reserves equation adds current total reserves to this list of variables, and the federal funds equation adds both current total reserves and current nonborrowed reserves. With this ordering of the variables, the disturbance term in the first equation can be considered a shock to total reserves; that in the second equation, the supply (or *policy*) shock; and that in the third equation, the demand shock. However, we dispense with this interpretation because this model omits any goal variable and is thus not capable of identifying policy shocks. If parameter values in VARs with this ordering are found to be similar for different definitions of period length, then VARs with other orderings must also be similar. But if parameter values are sensitive to period length, then, generally, parameter values will also be sensitive to period length if the ordering of the variables is changed.

We compare VARs in which the length of the time period is 2 weeks and 12 weeks.[8] In each case, the VAR estimated is

(16) $$TR^*_t = \alpha_0 + \sum_{l=1}^{L} \alpha_{1l} TR^*_{t-l} + \sum_{l=1}^{L} \alpha_{2l} NR^*_{t-l} + \sum_{l=1}^{L} \alpha_{3l} FF^*_{t-l} + \varepsilon_t$$

(17) $$NR^*_t = \beta_0 + \sum_{l=0}^{L} \beta_{1l} TR^*_{t-l} + \sum_{l=1}^{L} \beta_{2l} NR^*_{t-l} + \sum_{l=1}^{L} \beta_{3l} FF^*_{t-l} + \zeta_t$$

(18) $$FF^*_t = \gamma_0 + \sum_{l=0}^{L} \gamma_{1l} TR^*_{t-l} + \sum_{l=0}^{L} \gamma_{2l} NR^*_{t-l} + \sum_{l=1}^{L} \gamma_{3l} FF^*_{t-l} + \eta_t.$$

This VAR is similar to (13)–(15), which include a finite number of lags and exclude the goal variable. The shocks ε_t, ζ_t, and η_t are uncorrelated with current, leading, and lagging values of themselves and each other and are uncorrelated with past values of TR^*_t, NR^*_t, and FF^*_t. The lag length L is 12 periods when period length is 2 weeks and 2 periods when it is 12 weeks. Therefore, 24 weeks of past data are used regardless of the period length.

We will assess whether time aggregation affects the dynamics of the three-variable model in two ways. One way is to see whether the restrictions on the coefficients implied by a close relationship between borrowed reserves and the federal funds rate (that is, $\gamma_{10} > 0$ and $\gamma_{20} = -\gamma_{10}$) and the Fed targeting borrowed reserves (that is, $\beta_{10} = 1$) are reflected in the estimated model, in VARs estimated using 2-week and 12-week data.[9] Since the three-variable model is distinct from the four-variable model in which these restrictions were derived, this test is a single-edged sword: finding that these restrictions are satisfied would suggest our interpretation is reasonable, but finding that they are not satisfied could be ascribed to differences between the three-variable model and the four-variable model. The estimates strongly support the restrictions, however, as shown in Table 1. Moreover, time aggregation does not affect the conclusion that the federal funds rate is driven by borrowed reserves and the Fed accommodates changes in total reserves.

Another way we assess whether time aggregation affects the dynamics of the three-variable model is by comparing the impact of the same changes in total reserves, nonborrowed reserves, or the federal funds rate when the model is estimated using 2-week and 12-week data. To do this, we perform two sets of experiments to determine whether time aggregation is important in this three-variable model. We can think about our experiments in this way: Imagine that two economists use VAR models estimated with different period lengths to evaluate the effect of a shock to the three-variable model. Suppose that economist A uses 2-week average data and that economist B uses 12-week average data. Also suppose that both economists have observed the relation among the different variables in their models for some time.

To motivate the first set of experiments, suppose that both economists forecast the effect of total reserves being \$500 million higher than expected in every fine subperiod of $T + 1$. Economist A uses the 2-week data to solve for the set of shocks that induce total reserves to be \$500 million higher in every fine subperiod and then traces through the effects of these shocks using the VAR estimated with 2-week data. Economist B uses the 12-week data to solve for the shock that induced total reserves to be \$500 million higher in the coarse period $T + 1$ and then traces through the effect of this shock using the VAR estimated with 12-week data. The comparison of the two forecasts is the first experiment in this set. The other two experiments in the first set are motivated similarly: one begins with nonborrowed reserves being \$500 million higher than expected in every fine subperiod of $T + 1$, and the other one begins with the federal funds rate being 50 basis points higher.[10]

To motivate the second set of experiments, suppose that both economists forecast the effect of total reserves being \$500 million higher than expected in the last fine subperiod of $T + 1$. Economist A, who uses the 2-week data, solves for the shock that produced the increase and traces through the effect of the shock using the VAR estimated with 2-week data. Economist B, who uses the model with 12-week data, sees only that nonborrowed reserves have increased by an average of \$83.3 million over the 12-week period above their anticipated levels. This economist uses the model with 12-week data to solve for the shock that induced total reserves to be \$83.3 million higher in the coarse period $T + 1$ and then traces through the effect of this shock using the VAR estimated with 12-week data. The comparison of the two forecasts is the first of the second set of experiments. In the second experiment of this set, nonborrowed reserves are \$500 million higher than expected in the last fine subperiod of $T + 1$ (an apparent increase of \$83.3 million for the whole coarse period, to economist B), and in the third experiment the federal funds rate is 50 basis points higher than expected in the last fine subperiod of $T + 1$ (an apparent increase of 8.33 basis points, to economist B).

The Appendix provides the technical details of all the comparisons. The two sets of three experiments each are summarized in Table 2.

These two sets of experiments capture two alternative assumptions about the movements in the three variables. Before conducting the experiments and interpreting the results, we should consider how realistic the assumptions are. In the first set of experiments, the movement is *sustained* in that it persists for 12 weeks; in the second set, the movement is *brief* in that it persists for only 2 weeks. Some evidence on actual movements is provided by Charts 4–6, which show data for the three variables manipulated in the experiments. These data suggest that movements in all three variables are more like the first set of experiments in the sense that, except for short periods, movements in all three variables are relatively smooth. Another indication that the movements in these variables are relatively smooth is the fact that the variance of each of these variables sampled on a 2-week average basis is within 5 percent of its variance sampled on a 12-week average basis.[11]

Data and Estimation

Data measured every two weeks are the finest-frequency data available because, during our sample, data on total reserves and nonborrowed reserves were published only during each 2-week maintenance period—the period over which banks must, on average, meet their reserve requirements. Our sample period begins in October 1982, when the Fed switched to borrowed-reserves targeting, and ends in March 1993. We use three primary series: total reserves, nonborrowed reserves, and the federal funds rate. Following the practice in similar studies, we use data on total reserves and nonborrowed reserves that have been seasonally adjusted and adjusted for changes in reserve requirements.[12]

Since each 2-week maintenance period starts on a Thursday and ends on a Wednesday, our 2-week data for the federal funds rate are created by averaging the effective daily federal funds rate over the maintenance period. A daily rate immediately preceding *m* nonbusiness days is given $m + 1$ times the weight of a daily rate for a business day.

As Strongin (1992) does, we examine the relation among growth in total reserves, the ratio of nonborrowed reserves in the current period to total reserves in the previous period, and the average effective federal funds rate. Of course, when we look at coarser-frequency data (such as 12-week data), we compute level averages before computing growth rates.

We restrict ourselves to data after October 1982 for reasons discussed earlier. The period after October 1982 represents one policy regime. We think that the coefficients in VAR models such as the one we estimate here are subject to the Lucas (1976) critique and are not structurally stable across regimes. In fact, Strongin provides compelling evidence in this context that VARs were unstable across monetary policy regimes in the past two decades. As a consequence of this instability, we think that the effects of time aggregation on estimates of the effects of monetary policy should only be evaluated within a particular policy regime. Thus we restrict our data sample to start with the onset of the most recent monetary policy regime.[13]

Results and Interpretation

The results of the first set of experiments are shown in Chart 7; the results of the second set, in Chart 8. Each graph in these charts compares the model's estimates of a variable's response, over the next two years, to a shock to itself or to another variable when the model uses 2-week data and when it uses 12-week data. Each graph also shows a 90 percent confidence interval for the 12-week response. (The confidence intervals were estimated using the resampling method described in the Appendix.) In these charts, the graphs for total reserves and nonborrowed reserves show the predicted effect of each experiment on growth in those variables,[14] while the graphs for the federal funds rate show the predicted effect of each experiment on the federal funds rate itself.

In Chart 7, for example, the graph in the first row and column compares the response of total reserves to the shocks to total reserves described as *experiment 1* in Table 2. Note that in this graph there is little economically important difference between the model's estimates of the response of total reserves with 2-week data and with 12-week data and that the estimates are close to each other relative to the confidence bounds. The remaining graphs in Chart 7 tell a similar story. For experiments 1–3, there is little economically important difference between the estimates with the 2-week data and the 12-week data of the response of any variable in any of these three experiments. Thus the overall pattern that we see in experiments 1–3 is that time aggregation has little effect if the change in the affected variable is sustained, in the sense that the unanticipated change is the same for each of the six 2-week periods following the sample period.

Experiments 4–6 differ from experiments 1–3 in that the unexpected change with the 2-week data occurs only in the sixth 2-week period after the end of the sample. Since the average unanticipated change over the entire 12-week period is much different from the unanticipated change in the sixth 2-week period, these experiments can show us whether the brief changes cause problems with time aggregation. Chart 8 shows that time aggregation matters in measuring the effects of brief changes in total reserves or the federal funds rate, but it is only marginally important for nonborrowed reserves.

If data showed that the variables of interest behaved quite differently on a 2-week basis than on a 12-week basis, then we would rely on experiments 4–6 and conclude that time aggregation caused substantial problems in evaluating monetary policy. However, Charts 4–6 show that the variables considered in our experiments usually move fairly smoothly. As a consequence, we think that experiments 1–3 more accurately address the likely effects of time aggregation than do experiments 4–6. Since time aggregation has very little effect on experiments 1–3, we doubt that time aggregation is important in assessing the dynamic impact of changes in monetary policy.

Conclusion

We have compared the identification and evaluation of policy using a model estimated with data averaged over two different period lengths: 12 weeks, which is close to quarterly, the data frequency researchers use most often, and 2 weeks, which is the finest time period for which data on all the variables of interest are available. We find that aggregation from 2-week to 12-week periods has no effect on policy identification in our model.

The variables in our model are total reserves, nonborrowed reserves, and the federal funds rate. Regardless of the model's time period length, the Fed is completely accommodating, supplying nonborrowed reserves one for one in response to contemporaneous movements in total reserves. And regardless of period length, the federal funds rate responds only to borrowed reserves. Dynamics are not much affected by the use of 12-week data instead of 2-week data. Movements in total reserves, nonborrowed reserves, and the federal funds rate typical of those actually observed in the 2-week data have similar effects with or without time aggregation. As a result, time aggregation is not a problem within our three-variable model.

But does this conclusion have anything to say about whether time aggregation is significant in the four-variable model that interests us most? If time aggregation were a significant problem in the three-variable model, then it would certainly be a significant problem in the four-variable model. However, even though time aggregation does not appear to be a problem in the three-variable model, we cannot prove that time-aggregation problems do not exist in the four-variable model. But we think that such problems are unlikely, precisely because the goal variable is measured less frequently than other variables. If policy is based on observation of the goal variable, then policy itself must change slowly. If policy changes slowly and is persistent, then time aggregation will likely cause little problem when economists interpret the effects of monetary policy.

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[1]Here and hereafter, we use *banks* to mean all depository institutions required to hold a portion of their deposits in reserve at Federal Reserve Banks or as vault cash. These institutions currently include commercial banks, mutual savings banks, savings

and loan associations, credit unions, agencies and branches of foreign banks, and Edge Act corporations.

[2]In total reserves and nonborrowed reserves, we induce stationarity by dividing by the level of total reserves in the previous period. We assume that the federal funds rate is stationary, so no normalization is necessary. Normalization of the goal variable is a moot point, since we never actually estimate a system including a goal variable.

[3]This procedure also could be undone by the Lucas (1976) critique. That is, the coefficients in equations (3), (7), and (9) could change whenever the policy rule represented by (8) changes. However, since our focus is on how time aggregation affects policy identification—an earlier step in the procedure than policy evaluation—the Lucas critique does not apply.

[4]Some of the approaches used and examples of each are as follows: Event analysis (Romer and Romer 1989, 1990); nonstructural vector autoregressions (VARs), which are not designed to be invariant to policy regime changes (Strongin 1992); structural VARs, which are designed to be invariant to policy regime changes (Leeper and Sims 1994); traditional general equilibrium models with detailed financial sectors (Gilles, Coleman, and Labadie 1993); and real business cycle models with an appended monetary sector (Christiano 1991). More complete references can be found in recent papers by Gordon and Leeper (1994), Hoover and Perez (1994a,b), and Romer and Romer (1994).

[5]One issue in the literature is whether an easing in Fed policy would also increase inflationary expectations and thereby lead to an immediate rise in the interest rate. That seems implausible in our model because ours separates a maintained easing in policy, measured by an increase in a_{30}, and a temporary easing, measured by an increase in ε_{3t}. Thus an increase in ε_{3t} need not raise inflationary expectations.

[6]The reserve maintenance period changed from weekly to biweekly starting in February 1984. This change accompanied the switch from lagged reserve accounting to contemporaneous reserve accounting. However, after this switch, the essential policy of the Fed remained borrowed-reserves targeting, just as it had been since October 1982.

[7]We use 12-week data for our comparison, because 12 is an even multiple of 2, which facilitates comparison with the model using 2-week data.

[8]We also examined results for the model using 4-week data. This comparison is qualitatively similar. We do not reproduce those results here since we think that if time aggregation is important, it will show up clearly in a comparison of estimates of the model using 2-week and 12-week data.

[9]These restrictions correspond to the restrictions $a_{42} > 0$, $a_{43} = -a_{42}$, and $a_{32} = 1$ for the model described by equations (7)–(10).

[10]We choose this method of evaluating the effect of monetary policy in our sample period because policy during this period caused the targeted federal funds rate to change by multiples of 25 basis points and persist at its new level for an extended period. This discreteness and persistence in the federal funds rate cannot be completely captured by a linear VAR. As a consequence, we think that imposing this persistence in a conditional forecast experiment will allow us to more accurately evaluate the effects of time aggregation than if we relied solely on comparing the impulse response functions of the model using 2-week and 12-week data.

[11]If the value of a variable was identical in every 2-week period within a given 12-week period, then these two variances would be the same.

[12]In addition, like Strongin's (1992) data, they are adjusted for borrowings made to deal with specific financial crises, since these borrowings do not represent changes in monetary policy. We thank Steve Strongin for the data needed to make these adjustments.

[13]Some researchers think that policy regimes switched in February 1984 with the switch to contemporaneous reserve accounting. We found no significant difference in the estimated impulse responses of our model if our estimation period started in February 1984. We therefore use data starting in October 1982 to make our tests more powerful.

[14]As explained in footnote 2, growth for both total reserves and nonborrowed reserves is measured relative to the level of total reserves in the previous period.

Appendix

Testing for Time-Aggregation Effects

Here we describe the procedures behind the time-aggregation experiments discussed in the preceding paper.

In these experiments, we want to compare the impulse response functions from a vector autoregression (VAR) estimated at the highest available frequency with a model estimated with time-averaged data from fine periods. Let us denote vector data of dimension k from the coarser sampling frequency as X_t^*, $t = 1, ..., T$, where $X_t^* = (1/S)\Sigma_{s=1}^{S} X_{t,s}^*$ and $X_{t,s}^*$ is the vector value higher-frequency sampled data for subperiod s of t. Comparable models for the two sets of data can be estimated as follows. For the coarse-sampled data, if l lags are used, the regression will be

$$\text{(A1)} \quad A_0 X_t^* = A_1 + A_2 X_{t-1}^* + A_3 X_{t-2}^* + ... + A_{l+1} X_{t-l}^* + \varepsilon_t$$

where A_0 is lower triangular and the covariance matrix of ε_t is diagonal. Equation (A1) can be rewritten in strictly autoregressive form as

$$\text{(A2)} \quad X_t^* = C_1 + C_2 X_{t-1}^* + C_3 X_{t-2}^* + ... + C_{l+1} X_{t-1}^* + \nu_t$$

where $C_i = A_0^{-1} A$ and $\nu_t = A_0^{-1}\varepsilon_t$. To use data from the same periods to estimate the dynamics of the finer-sampled data, $l{\cdot}s$ lags would be included in each regression; that is, the regression would be

$$\text{(A3)} \quad B_0 X_{t,1}^* = B_1 + B_2 X_{t-1,s}^* + B_3 X_{t-1,s-1}^* + ... + B_{s+1} X_{t-1,1}^* + B_{s+2} X_{t-2,s}^* + ... + B_{ls+1} X_{t-l,1}^* + \varepsilon_{t,1}$$

where B_0 is lower triangular and the covariance matrix of $\varepsilon_{t,1}$, $V(\varepsilon_{t,1}) = V_k$, is diagonal. Equation (A3) can be rewritten in strictly autoregressive form as

$$\text{(A4)} \quad X_{t,1}^* = F_1 + F_2 X_{t-1,s}^* + ... + F_{ls+1} X_{t-l}^* + \nu_{t,1}$$

where $F_i = B_0^{-1} B_i$ and $\nu_{t,1} = B_0^{-1}\varepsilon_{t,1}$.

Now suppose we want to perform the following experiment. If we assume that the vector model is at its long-run equilibrium as of T, what is the effect on the forecast of each model for periods $T + 2$ through $T + P$ of a given set of deviations of the ith component of $X_{T+1,s}^*$ for each of the S subperiods of $T + 1$? Suppose we call this set of deviations δ_s, $s = 1, 2, ..., S$. The answer to our question is easy for the coarse model. The average unexpected deviation to the ith component of X_{T+1}^* is $\bar{\delta} = (1/S)\Sigma_{s=1}^{S}\delta_s$. Therefore, to analyze the effect of this unexpected deviation, we would shock the ith equation of (A1) by $\bar{\delta}$ in period $T + 1$ and examine the resulting dynamics for the next $P - 2$ periods when compared to the unconditional forecast of (A1). Examining the effect of shocks in (A3) is a bit more complicated. We have already specified the set of deviations of the ith component of $X_{T+1,s}^*$. Now we need to specify how we compute the shocks to (A3) that produce those deviations.

Let us define the following:

δ_s = the deviation of the ith component of $X_{T+1,s}^*$ from its unconditional forecast.

D_s = a $k \times 1$ vector of zeros, except for the ith row, which is δ_s.

G = a $1 \times k$ vector of zeros, except for the ith column, which is one.

$\hat{X}_{T+1,s}$ = the unconditional forecast of $X_{T+1,s}^*$, given information at $X_{T,s}^*$.

$\tilde{X}_{T+1,s}$ = the conditional forecast of $X_{T+1,s}^*$, given information $X_{T,s}^*$ and the sequence of shocks needed so that $G(\tilde{X}_{T+1,s} - \hat{X}_{T+1,s}) = \delta_s$.

γ_s = the shock to the ith equation of (A3) needed so that $G(\tilde{X}_{T+1,s} - \hat{X}_{T+1,s}) = \delta_s$.

$Z_s = B_0^{-1} G' \gamma_s$ = the shock to (A4) needed so that $G(\tilde{X}_{T+1,s} - \hat{X}_{T+1,s}) = \delta_s$.

$\lambda_s = (\tilde{X}_{T+1,s} - \hat{X}_{T+1,s})$ = the difference between the conditional and unconditional forecasts of $X_{T+1,s}^*$. (Note that $G'\lambda_s = \delta_s$.)

In period $X_{T+1,1}^*$, $\gamma_1 = \delta_1$. In the subsequent subperiods of $T + 1$, we need to solve for γ_s because of the dynamic effects of the shocks $\gamma_1, ..., \gamma_{s-1}$.

For computational simplicity, we will solve for γ_s using (A4), which is isomorphic to (A3). Of course, the γ_s from (A4) would be exactly the γ_s used with (A3).

Note that $\gamma_1 = \delta_1$, so that $Z_1 = B_0^{-1} G' \delta_1$ and $\lambda_1 = Z_1$, for $s = 2, ..., S$, and

$$\text{(A5)} \quad \gamma_s = \delta_s - \sum_{j=1}^{s-1} G' F_{j+1} \lambda_j$$

$$\text{(A6)} \quad Z_s = B_0^{-1} G' \gamma_s$$

$$\text{(A7)} \quad \lambda_s = Z_s + \sum_{j=1}^{s-1} F_{s+1-j} \lambda_j.$$

The effects of time aggregation on the impulse response functions can be seen by computing the difference between the average conditional forecast in (A3) for periods $T + 1$ through $T + P$ and the unconditional forecast and comparing that difference with the same computation for model (A1).†

Of course, the point estimate of the response to a given shock in each of the two models does not give us any indication about how uncertain we should be about the estimated impulse response functions. We use 1,000 replications of bootstrap resampling to compute confidence intervals for our impulse response functions using the method suggested by Runkle (1987). That method initially estimates a VAR model and then generates artificial data sets by drawing from the estimated residuals (with replacement) and generating new data using the estimated regression parameters and the initial values of the data for the number of lags in the regression. After each artificial data set is generated, a VAR is estimated. The conditional impulse response functions described above are also computed for each data set. The graphs we present show the centered empirical 90 percent confidence intervals for conditional impulse response functions from the bootstrap regressions.

†Of course, if some of the *X*s are growth rates, as is true in our model, the average-level forecast for each set of coarser-frequency data should be computed directly from the finer-frequency data for (A3). Those average-level data should then be used to compute the growth rates for the coarser-frequency data generated by (A3) that are comparable to those in (A1). Using average differences in fine-frequency growth rates for the comparison would be invalid.

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Table 1

The Estimated Coefficients

Estimated Using Two Different Data Frequencies*

Data Frequency	Contemporaneous Total Reserves Coefficient in Equation for Nonborrowed Reserves β_{10}	Contemporaneous Total Reserves Coefficient in Equation for Federal Funds Rate γ_{10}	Contemporaneous Nonborrowed Reserves Coefficient in Equation for Federal Funds Rate γ_{20}
2 Weeks	.992 (.045)	47.2 (18.7)	–52.1 (18.1)
12 Weeks	.992 (.048)	31.8 (6.91)	–27.5 (6.40)

*Coefficients are estimated using U.S. data from October 1982 to March 1993.
Numbers in parentheses are standard errors.
Source of data: Federal Reserve Board of Governors

Table 2

The Experiments

How the Variables Are Shocked, Compared to the Model's Unconditional Forecast

Type of Shock	Experiment	Variable Shocked	Amount and Length of Shock for Each Data Frequency: *Fine:* 2-Week Periods	Amount and Length of Shock for Each Data Frequency: *Coarse:* 12-Week Periods
Sustained	1	Total Reserves	Up $500 million for six 2-week periods	Up $500 million for one 12-week period
	2	Nonborrowed Reserves	Up $500 million for six 2-week periods	Up $500 million for one 12-week period
	3	Federal Funds Rate	Up 50 basis points for six 2-week periods	Up 50 basis points for one 12-week period
Brief	4	Total Reserves	Up $500 million for just the last of six 2-week periods	Up $83.3 million for one 12-week period
	5	Nonborrowed Reserves	Up $500 million for just the last of six 2-week periods	Up $83.3 million for one 12-week period
	6	Federal Funds Rate	Up 50 basis points for just the last of six 2-week periods	Up 8.33 basis points for one 12-week period

Charts 1–3

The Effects of a Positive Shock to the Demand for Total Reserves

In the Market for Nonborrowed Reserves

Chart 1

If the Fed Does Not Accommodate

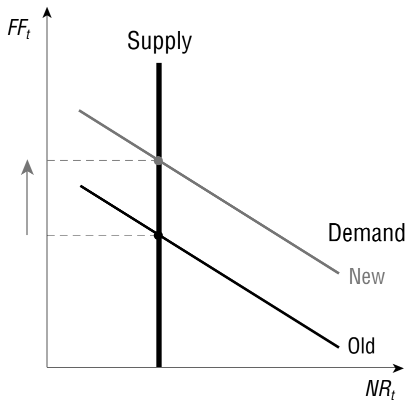


Chart 2

If the Fed Accommodates Partially

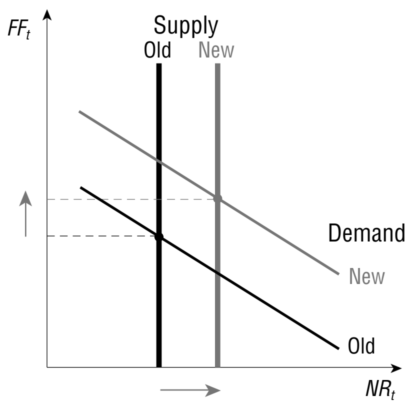
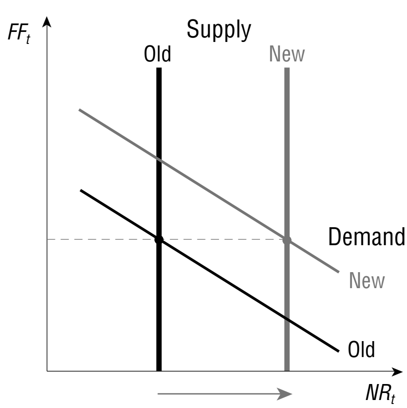


Chart 3

If the Fed Accommodates Completely



Charts 4–6

Actual Data for the Three Key Variables

Biweekly, From 1982 (October 13) to 1993 (March 31)

Chart 4 Total Reserves*

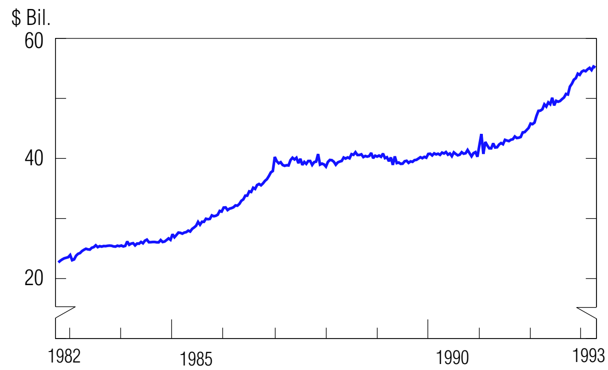


Chart 5 Nonborrowed Reserves*

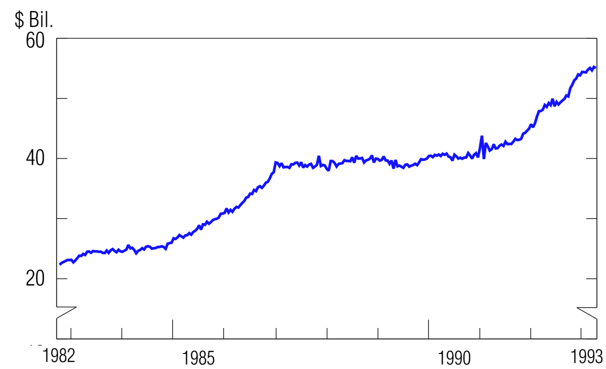
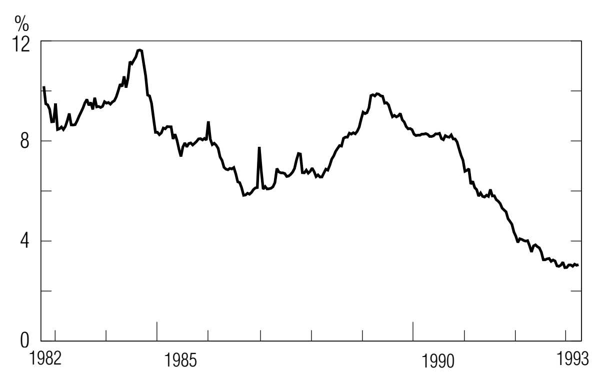


Chart 6 Federal Funds Rate**



*The reserves data are for all depository institutions and are adjusted for seasonal variation, changes in reserve requirements, and borrowings made to deal with specific financial crises.

**The federal funds rate data are averages of daily rates during each 2-week period.

Sources: Federal Reserve Board of Governors and Steve Strongin

Chart 7 **Experiments 1–3: Responses to Sustained Shocks**

How Each Variable Responds to Shocks to Each of Them (as Described in Table 2)

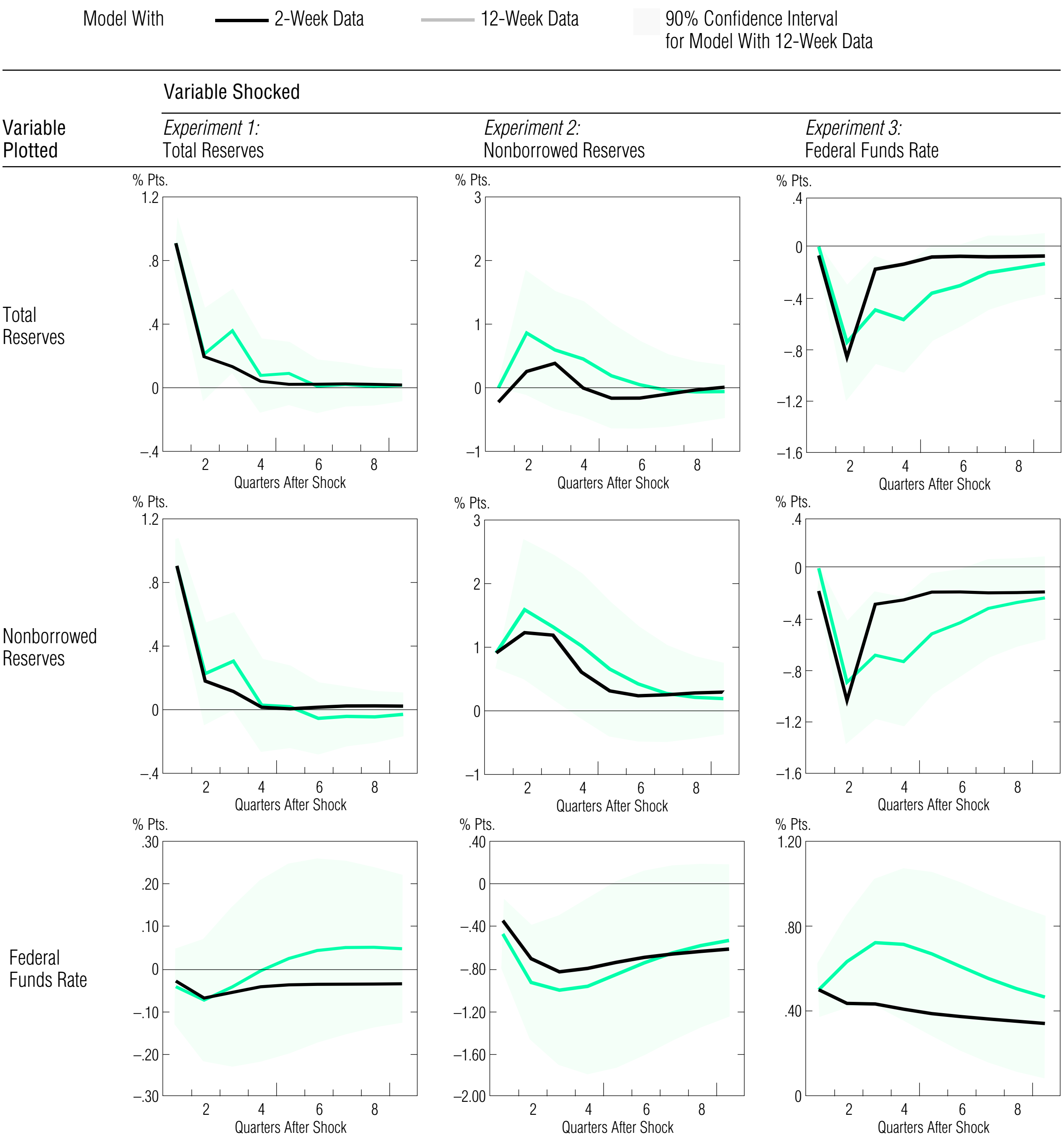


Chart 8 **Experiments 4–6: Responses to Brief Shocks**

How Each Variable Responds to Shocks to Each of Them (as Described in Table 2)